

1992/17

/BROWN

AN
ATTEMPT TO ESTIMATE
THE
POWER OF MEDICINE
IN
CONTROLLING FEVER.

BY
WILLIAM BROWN, M.D.

FELLOW OF THE ROYAL COLLEGE OF SURGEONS, AND EMERITUS
SURGEON OF THE ROYAL INFIRMARY.

Εθεμελίωσας την γῆν καὶ διαμενεῖ.
Τῇ διάλᾳξεί σου διαμενεῖ ἡμέρᾳ ὅτι
Τὰ συμπαντα δοῦλα σά.
Ὑμνος εἰς Δαβίδ.

EDINBURGH:

PRINTED FOR WILLIAM WHYTE, ST ANDREW'S STREET, AND
ADAM BLACK, SOUTH BRIDGE, EDINBURGH;
LONGMAN, HURST, REES, ORME, & BROWN; T. & J.
UNDERWOOD; AND ANDERSON & CHASE,
LONDON.

1818.

34 7966



TO

JAMES HAMILTON, M. D.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, AND
SENIOR PHYSICIAN TO THE ROYAL INFIRMARY;

AND TO

ANDREW WOOD, Esq.

SENIOR FELLOW OF THE ROYAL COLLEGE OF SURGEONS.

MY DEAR FRIENDS,

Pardon me for having prefixed your names to the following pages. I have done so to testify my regard, and to acknowledge how much I owe to you for the steady and unremitting kindness with which you have softened my own long continued personal distress, and that of several members of my family. We have indeed experienced the value of Medicine, when under the controul

of a sound understanding, and administered by the hand of friendship. While I am not forgetful of what I owe to my other Friends, and their attentions have not been few, suffer me to thank you thus publicly, and to beg your acceptance, with your usual goodwill, of the following Notices, which are not written for your instruction, but which may perhaps have some effect in repressing that arrogance of science, which has betrayed many into a belief, that they have learned to accomplish purposes which are still placed beyond the reach of human attainment. I am,

My dear Friends,

With esteem and respect,

Your humble Servant,

WILLIAM BROWN.

EDINBURGH, 46. Hanover-street.

March 12. 1818.

PREFACE.

AMONG a very great number of the Community, our profession is not held in the estimation which its usefulness has a right to claim. Men in good health are by no means displeased to have *a fling at the Doctors*, and are not shy in expressing a doubt at least of the efficacy of medicine in removing disease. Nothing is a more evident proof that it does not possess that confidence which it certainly deserves, than this, that cautious calculating men overlook the advantages which it affords. The framers of schemes for life annuities never take the efficacy of medical advice into their account. They indeed inquire whether the person who proposes to deal with them has

any thing about him that has a tendency to shorten life ; but they do not make it a stipulation, that he shall send for a Physician when he is sick.

It is easy to account for all this, and to trace it to the fault of the profession themselves, or rather to the misfortune of many of them.

Physicians may be said frequently to promise too much, and too often to insinuate some personal superiority of skill in the cure of disease. Mankind see daily, however, that their skill fails them ; and it is a fact too notorious to be concealed, that few, if any of the human race, die of old age. It follows almost, of course, that the art is tacitly branded with the disgraceful name of pretension.

It were much to be wished, then, that the real boundaries of the usefulness of Physic could be fixed, and that it were made evi-

dent to the common sense of mankind how far they may safely trust to its assistance.

With this view, I have endeavoured to inquire into the efficacy of Medicine in the cure of Fever. I trust I have succeeded in showing, what indeed is openly expressed by some part of the profession, and suspected by almost all men, that we cannot stop the course of this disease. But I do not wish to be misunderstood ;—while my conviction of this truth is firmly established, I am not less convinced that the due application of Medicine most certainly renders Fever more easily borne, and prevents death in many instances. Providence has established as certain a connexion between the means and end in this case, as in any of the operations that take place in the material world. If Physicians sometimes fail in their attempts to cure disease, they ought no more to be blamed than the husbandman who fails to produce a good crop. In both cases, all the necessary means are not

put under human controul ;—in both cases Providence has retained these in his own hands ;—and in both cases the necessity of suffering is imposed on mankind.

AN

ATTEMPT, &c.

ABOUT the end of the last century I endeavoured, from the examination of the records of an hospital, to form an estimate of the efficacy of medicine in the cure of Fever. My inquiries led me to conclude, that the means employed did not shorten the duration of this disease, and I endeavoured to maintain the truth of this opinion in a paper which was published in the 2d vol. of the 2d lustrum of the Annals of Medicine.

Since I came to this conclusion, the world has become somewhat older, and may be supposed to have acquired more knowledge

by the opportunities which a longer continued experience has afforded. The Medical Profession have not been idle spectators of what was passing around them ; among other things, the subject of Fever has been made a particular object of inquiry, and the questions of its origin, and of its connexion with local inflammation, have occupied the attention of many well informed men. Indeed, the destruction which, during these twenty-five years, Fever has produced in many parts of the world, has roused, and at the same time justified, the exertions that have been made in its investigation.

Knowledge is said to be power. Real medical knowledge, then, must be understood to mean, a power of preventing and curing disease. It is by this test that I would wish to consider the advancement that has been made in our knowledge ; and to determine, whether the power which we possessed of preventing and curing Fever at the time I wrote my first paper is now really increased.

The means of preventing Fever have been accommodated to the avoidance or destruction of its supposed causes.

Besides the circumstances that are observed to derange the health, and that are therefore thought to render the body more liable to Fever, Physicians have presumed three sources or causes of this disorder, *viz.* a particular constitution of the material world ; the vapour that is produced by heat and moisture ; and a matter which is usually called contagion.

I am very doubtful whether our knowledge on these points has been increased within these twenty years. I am certain, however, that it is not even now sufficient to enable us to say that our power over Fever is greater than formerly. We are unacquainted with the nature of the condition of the material world, and of its atmosphere, which produces sickness ; nay, some Physicians doubt if such a condition exists. But among those who believe it, the pious mind looks up to the Author of nature as having sent it for good and wise purposes ; and, humbled with a view of his own weakness and ignorance, depends on Him for its removal. The Philosopher, who wishes to explain all things, guesses at its nature, and endeavours to trace it in a

chain of sequences, which, if he follow to the termination, will ultimately lead him to the same source. The practical value of the mode of thinking of both is, however, precisely equal, as far as regards the prevention of disease. It is absolutely null. What may be the value in other respects, it is not my office to ascertain.

Miasmata of marshes were long deemed an exclusive source of a particular kind of Fever; we have now, however, added the vapours extricated during the decomposition of animal and vegetable matter, in any situation. Our knowledge in this matter may be said to have been increased during the period I speak of; but a corresponding power of preventing Fever, which such increase of knowledge might be supposed to afford, has not accompanied it. The events that have taken place in the West-Indies, in America, in Spain, at Walcheren, and elsewhere, have plainly proved that our knowledge in this matter has not conferred any useful degree of power. Indeed, it is to be remarked, that the production of marsh miasmata of greater activity than usual, is always connected with some con-

dition of the material world, or of the affairs of men, which we possess no means of controlling. Our knowledge then of this matter, is of very little practical utility.

Our acquaintance with contagion in its mode of production and in its nature, in my apprehension, is not increased ; neither is our power over it extended.

That we know not what contagion is, will readily be granted ; that our controul over it is very considerable, will be contended for by many ; and that this controul is rendered more complete within these twenty or thirty years will be asserted by a few.

The controul we have over contagion must arise from the power we have of preventing its production, and from the means we possess of destroying it when produced. In this instance, knowledge, many presume, has brought power along with it.

But it must be remarked, that the knowledge of the source of contagion which was considered by the generality of Physicians as perfect, has been strongly attacked of late, and in such a way as to lead us to entertain doubts whether our notions concerning it were correct. We are then rather

thrown back in this matter, and made to hesitate whether we really possess that degree of the power of preventing contagion which we thought we had acquired.

Without attempting to establish what is the origin of the contagion of Fever, a thing I candidly confess myself unequal to, I shall only just mention a fact, which in my mind proves one of two things, either that we do not enjoy means adequate to the prevention of the spreading of Fever, or that we do not employ them.

Fever is a disease that exists in Edinburgh every year, in a certain degree. I think, however, I may say, that for twenty years past we have heard very little of it; it did not spread among the inhabitants, nor cause any alarm. Very little, if any thing, was said of the propriety of using means for stopping its progress, or for its extinction. Certainly families were left to secure themselves, without more than ordinary professional and domestic exertions. The few people affected with it, either died or recovered, without noise; and the city was considered as healthy.

For a good many months past, however,

we are sure that Fever has been more frequent. Nay, so much so, that professional men have considered it their duty to call the public attention to it, and to exhort and encourage them to put in practice what are counted the most effectual means of arresting its progress ; and we have every reason to believe that the means are assiduously applied : At all events, we are sure that more is done in this way now than in former years ; yet the Fever continues, and we shall soon, I doubt not, have ample notice of the many hundreds, perhaps thousands, who have been cared for by the public institutions.

We may thank Heaven that the disease has been so mild hitherto. We ought to thank the benevolent individuals who bestow their time and labour in looking after the sick ; but the continuance and spread of the evil shows too plainly that the power over contagion, which our knowledge of its origin has conferred on us, is very inconsiderable. Indeed, the acknowledged fact that the Fever has appeared in houses that had undergone purification, would warrant a stronger conclusion.

It is asserted that means have been discovered which are capable of destroying contagion where it already exists. Indeed, these means have been reckoned so sure, that a public reward was given to the meritorious individual who discovered them. The means to which I allude will easily be known to be the mineral acids in a state of vapour or gas. The knowledge of their efficacy would indeed be a valuable discovery, and would certainly, were it established, confer power ; but unluckily the fact does not appear to have been satisfactorily ascertained. Indeed, if what some of our transatlantic brethren say be true, we might hesitate a good deal before we admitted the evidence by which it is attempted to be established. Some of the Americans allege that contagion is of an acid nature, and can only be destroyed by alkalies. Now, it is at least not very probable, that the same noxious matter can be neutralized, or rendered inert, with respect to its effects on animal bodies, by substances of opposite qualities.

But besides this, we have very respectable testimony to assure us, that the acid gases

have not the effect imputed to them ; and, indeed, I do not see that in the present instance, as was noticed in a former paragraph, any means have been effectual in checking the progress of the present Fever. I am much in doubt, therefore, whether this discovery has added any thing to our knowledge of contagion, or has increased our power over it.

While I have thus attempted to show that the power over contagion, derived from an increase of our knowledge, is very limited, I by no means wish to inculcate the doctrine of tame submission, or that it should be understood that we are not to struggle against its baneful influence. We certainly do possess means which, when duly applied, are of use to us. These are not, however, founded on any newly acquired knowledge of contagion, but on that knowledge which we formerly possessed, which is almost instinctive to every thinking person, or which may be easily obtained by any human being.

It may seem foreign to my expressed purpose in coming before the Public, to enter into any detail of the means we for-

merly possessed for preventing or suppressing contagion. My avowed intention is, to examine whether our knowledge of this, and the nature of Fever, has given us any increased power to defend ourselves. But it may be useful to the younger part of the profession to call their attention to common-place things, since the ardent mind of youth is very apt to occupy itself with the fascinating discoveries of scientific research, while the truths which are the offspring of common observation are less esteemed, and less made the foundation of practical operations.

The most obvious means of stopping the spreading of a contagious disease, is to separate the sick and the healthy. This commends itself to every understanding, and is of course universally adopted as far as possible. Houses have been provided in many places for the reception of the infected poor, and the most beneficial consequences have been said to attend this mode of security.

The advantages of separation are so evident, that it surely would be absurd to deny their existence. Yet we all know how much

things are often over-rated, and it would be desirable, were it possible, to ascertain the real value of this expedient. Certainly more has been ascribed to it than it deserves. It has been held out in some instances as having actually stopped the progress of contagious Fever, and the safety of large cities has been thought to be owing to its employment. This opinion is founded in error; the history of contagious diseases plainly shows its fallacy. In no instance has a contagious ailment affected a whole population. In no instance has it been propagated for ever. In every instance on record, or within our own memory, it has ceased, although no fever-houses were provided, nor any other extraordinary means employed. Nay, it is farther certain, that contagious Fever almost always exists in large cities; yet it is only on particular occasions that the infection spreads.

Now, although the knowledge of these facts should not induce us to forego the advantages that may be derived from providing fever-wards for the sick poor, yet the community should not be dismayed if it be found that such provision cannot be so ex-

tensively procured as some might wish ; and the more so, because this good cannot be obtained without a certain share of concomitant evil.

Without going farther in the consideration of this, which may be called one of the special means of suppressing a contagious disease, I shall content myself with reminding the young of a very few general truths.

According to a rule of conduct almost instinctive, we shun any near connexion with a person in Fever, in as far as our felt duty to him, and to our own conveniences, admit. If we love the person sick, we also soon see, that the kindest office we can do him is to leave him much to himself, and avoid harassing him with unnecessary attentions, and with endeavours to supply him with the intercourse and communications which interest and delight a person in health. Guided by this simple appeal to feeling for ourselves and for the sick, we shall thus expose ourselves as little as possible, consistently with our duty, to the immediate source of contagion.

The same natural guide leads us to the knowledge of the necessity of a proper sup-

ply of food, if we mean to live in health. It appears to be of very small importance what kind of food we use, provided it be agreeable to the palate and in sufficient quantity.

This is, indeed, a common-place remark. Every body knows that we cannot subsist without food. But the importance of it will readily appear when we reflect, that science can supply no substitute. Fever-wards, fumigations, ventilation, and cleanliness, will, indeed, have little effect among a population worn down under the privation of due nourishment. Hence it is, that the companionship of famine and pestilence has become proverbial. It would, indeed, be contrary to fact to say that no person who has plenty of food will be affected with Typhous Fever; but I doubt much if it can be substantiated, that this disorder ever spread among a population fully supplied with the necessaries of life, protected from the effects of cold, unexhausted by immoderate labour, and the baneful influence of protracted debauchery. But if such be the fact, why should we call upon the Public to put confidence in means of precaution, that can

only be of any considerable usefulness when used in connexion with a pre-requisite condition, which it is not in our power to produce?

What is said of food may be applied to air. Health requires a due supply of good air. It is to be noticed, however, that air which is reckoned unhealthy is not always unpleasant to the person who uses it; at least this is the general opinion; and as I am not in possession of facts to prove the contrary, I must presume, as far as practice is concerned, that this general opinion is well founded.

But there are some sensible qualities of the air, which are said to be inimical to health, and favourable both to the production and propagation of Fever; and of some of these I shall take notice, more particularly because I think that many of the profession are misinformed concerning them.

I believe it is a certain fact, that the temperature of the air, considered in itself, does not affect its purity: Neither will it be denied that the air becomes impure in crowded places. If, then, in two rooms of equal dimensions and closeness, the one of which

being, however, moderately warm, and the other unpleasantly cold, the same quantity of defilement were generated in a given time, the air of these rooms in respect of purity would be the same ; is it not certain, however, that the persons in the cold room would suffer more than those in the warm ? Would not the debilitating power of cold be superadded to the effects of the impure air ?

From these facts, ought we not to be persuaded, that the common practice of keeping a little fire during the winter season, in the chamber of the sick poor, whose houses are usually small, for the production of a change of air in them, is both reasonable and salutary ; nay, much more so than the practice of constant open windows. The beneficial effects of summer heat on all animals, and the pleasurable feelings that always accompany warmth, convince my understanding at least, that the direction of many practitioners to keep the rooms cool, is often inconsiderately enforced, and that the practice followed by persons left to themselves is more rational, viz. to preserve their apartments in an agreeable heat.

There is however a very marked difference between a hot and a warm room ; the one gives pleasure, and the other uneasiness.

The excreta of the sick are commonly considered as a cause of defilement of the air. Whether this be the case or not, it is surely proper to have them speedily removed. They are offensive, and the uniform wish and practice of mankind, where opportunity offers, is to deposit them at some distance from their habitation. Here, by the bye, I cannot help noticing, that the acquisition of good very frequently cannot be enjoyed without some concomitant evil. Thus, our new Police system has provided for the cleanliness of our streets, but it has at the same time imposed on many of our citizens the necessity of keeping in their houses impurities which used to be more speedily disposed of. This must be a real hardship on very many. May it not be looked on as a collateral cause of disease, if unpleasurable feelings have any tendency to depress animal vigour ; a fact which every one who has felt them must acknowledge.

Connected with the purity of the air is the opinion generally entertained of the very great advantage of large apartments for the preservation of health and cure of disease. This opinion is the result of science, not of experience, and has established itself so firmly in the mind of the public, that in the construction of public, and even private edifices, it is a primary object to make them large and airy. If such edifices are so very necessary for the preservation of health, it leads to a very melancholy inference, because it is certain that a very small proportion of mankind only can procure such advantages. But it surely is worth while to inquire whether the fact be really so.

To bring this matter more under the cognizance of common understandings, suppose a large covered edifice, constructed with all the usual apparatus for changing the air in it. It surely is evident, that without special care and attention the place would soon become close and damp, the volume of air to be changed being so great. It appears also evident, that the influence of the sun must be excluded in a greater pro-

portion from large edifices than from small houses. The wholesome operation of this influence cannot be compensated by any means known. In proportion as the solar rays are excluded, the air approaches nearer to that of winter and night, and neither of these seasons are favourable to health. In our climate, also, other qualities of the air, besides that of not having been breathed, are found requisite for our well-being—the air must be artificially prepared before it is fit for use. If I may be allowed a homely phrase, our air must be cooked to be rendered wholesome. To oblige a poor man to use air which he cannot afford to prepare, is surely unfriendly. Indeed there is a very great risk that the rich, at least the middle class, will have this operation imperfectly conducted, if they attempt to do more than what is wanted for their comfort. It must be made a special business in the establishments of the opulent; and in this way may be effected. But in the less wealthy, the preparation of the air of their whole dwellings is rarely attempted, and even in the parts usually occupied, may be but indifferently done.

On the other hand, in a small cottage, the preparation of the air is easily conducted, and its complete change easily effected. Nay, as it is necessary to go in and out frequently, while at the same time a fire is burning in it, the air must be changed without any care on the part of those who occupy it. Large rooms, then, appear to me not to be of necessity the most airy places; these two properties have been inconsiderately associated.

The truth of this view of the matter will be confirmed to any one who has felt the annoyance produced, in a very large room, by the smell of dinner many hours after the repast is finished, even where attention has been paid to opening the windows. Another thing may also be observed, that in the summer season large rooms, even moderately filled, get unpleasantly hot and close independent of open windows. The most effectual mode of cooling such rooms, and giving them freshness, is to kindle a small fire in the chimney from time to time.

It is unnecessary, and would certainly be tiresome, to go over in detail all the things that the common sense of mankind has dis-

covered to be useful in promoting health and comfort. A little reflection will show how they bear on the present subject, and point out the superiority they possess in respect of efficiency over the artificial means that have been devised and recommended for preventing Fever.

It will readily occur to any one to say, though your observations are not new, yet they are just ; but what good purpose do they serve ? Can you not point out a more practicable plan of preventing Fever ? How is it possible to procure for the inhabitants of this city, those means which we all know are most conducive to the preservation of health and enjoyment ? This cannot be done ; and after all, you do not promise, and indeed we know that you cannot promise, that what you have pointed out as the best means of preventing Fever, could they be procured, will have the desired effect.

I must acknowledge, that indeed the prospect of preventing contagion, by furnishing the inhabitants of the city with the best means of preserving health and enjoying comfort, is in a certain degree Utopian.

We cannot make the whole population enjoy the advantages which in all ages have been restricted to a few. But it surely is desirable, if it be possible, that the public should entertain just notions on this subject, and have their exertions directed to a right channel. The expense of the administration of the artificial means of destroying contagion ;—the inconveniency and distress occasioned by the removal or separation of families ;—the annoyance occasioned by the chilling ventilation, damp winter white-washing, &c. of houses ;—the risk arising from the diffusion of acid gases in the apartments of the sick, which every body would say defiles the air, did not men of learning assure us otherwise ;—the alarm occasioned by the universal bustle and activity of the benevolent ;—and the well meant, but I suspect sometimes harassing labours of the Medical Profession, would be avoided ; while the safety of the Public would be equally well preserved. Sensible people should be told candidly, that Medicine has hitherto discovered no certain way of stopping Fever ;—that the most effectual means

are those which are dictated by the exercise of common sense ;—that the disease will take its course, and that we must wait the order which Providence has irreversibly established ;—that all that can be done is to keep the sick and well as much separated as the nature of the relations of society and of duty permit ;—that the effectual way to assist the sick poor is to minister to their common necessities ;—and the way to assist the poor in health, and prevent them from becoming sick, is to give them labour and to pay them for it. In this way more may be done than we think of. There are few houses, either among the rich, the middle class, or the labourers themselves, in which something is not to be mended, altered, or rendered somehow more convenient. To keep our domestic machinery in good order, affords a great deal of employment ; and were it the tone of the community to have this machinery always in good condition, it would afford still more. It is perhaps out of place to speak of such things at present ; and the more so because the public benevolence is in full activity to devise means for supplying the wants of our

population. Suffer me only to observe, how much useful clothing, and other necessities, we have occasion to believe, lie unemployed, and improvidently decaying in many of our lumber-rooms. Would it not be profitable to all to bring such things forth, and thus enlarge the market with useful and necessary commodities?

But does not benevolence sometimes overshoot the mark, and defeat its own aim? Feeling for the distresses of others is imposed upon us by the necessity of our natures. As we wish to remove every thing that is unpleasant, so there is reason to think we all endeavour to stifle this feeling, though with different degrees of earnestness, and by different means. Benevolence has devised an easy way of relieving us, in a great measure, from this irksome companion. She has contrived public charitable institutions, and devolved the care of them on a few, and established such rules of conducting them, that the end proposed may be accomplished, even although the persons immediately employed be divested of her characteristic feeling. From the crowd she only requires money, and this she gratefully receives in

exchange for the charity of the donor. He too frequently thinks he has thus purchased an immunity from what nature has imposed on him as a duty. He has contributed his share, and he is quit. But in this, as in all cases, when a man buys an indulgence from the performance of a duty, if he relieves himself from its irksomeness, he also deprives himself of the benefit that attends its fulfilment.

Public charities are certainly imperiously called for in the present state of the world—it is to be lamented that such a state exists. That they produce some good and much evil cannot be doubted. Among other evils this does not appear to be the least, that they weaken that mutual dependance which one man feels he owes to another, and thus deprive us of a great deal of the benefit of moral discipline.

I proposed further to inquire, whether our knowledge of Fever had increased, and had conferred upon us a corresponding increase of power in curing it?

Since the writing of my former paper, the knowledge of Fever does not seem to have been extended. Then, as now, we knew

that it was a disease of the whole system, frequently accompanied with local pain connected with inflammation. Frequently, however, we found Fever without any marked local pain ; and hence it was concluded that topical inflammation was not a necessary, but merely a casual symptom of this disease. In proportion however to its severity, the danger was estimated. This local affection was therefore much attended to by practitioners, and its presence was acknowledged to call for a mode of treatment considerably different from what was reckoned proper in other cases.

Of late years topical affections have either more frequently occurred, or have been more sought after, not only in Fever properly so called, but in some other febrile diseases. Indeed their occurrence has been found to be so frequent, that several Physicians consider inflammation as an essential part of Fever. I think I may venture to say that the truth of this position has not hitherto been satisfactorily established.

Though it may be doubtful whether our knowledge of Fever be increased, and our power over it, derived from this source, ex-

tended ; yet it has been asserted by some, and is still believed by many, that though no new remedies have been discovered, the extended application of the old has been made more effectual for shortening its duration, and lessening its danger. These pretensions I now beg leave to examine.

The first of these remedies, though of very ancient origin, was restored to notice, and even commended to very general estimation and use, by the late Dr James Currie, about the end of the last, or beginning of the present century. The third edition of his Work, in which a great number of favourable testimonies, and some few doubtful or unfavourable ones, are collected, was published in 1805. In my paper printed in the *Annals of Medicine* 1802, I took no notice of this remedy, because my inquiry extended only to the result of the practice of the Hospital to the records of which I had access. When cold-bathing was introduced into notice, the certainty of its power and efficacy was supported by all the authority of the great names of antiquity ; and its patrons ransacked the writings of the Greeks, Arabians, and Europeans, to find

testimonies in its favour. This search was not in vain, and its result was held forth to the world with sufficient pomp and triumph. The practice of nations called barbarous was adduced in proof of the legitimacy of its pretensions, and so far had the efficacy of cold-bathing, affusion, or spunging, established itself in the public opinion, that to doubt of the validity of its claims, or to decline its employment, required a considerable degree of professional reputation, and some share of personal firmness. It is now scarcely necessary to scrutinize its pretensions ; they were certainly overrated. It is falling into its former neglect, and is scarcely employed to the extent formerly recommended, by any person who judges soberly, who has seen much of disease, or who balances the comfort and ease of the sick under his care, with the harassment and inconvenience of the remedy.

It is neither illiberal, nor inconsistent with the known nature of man, to allege that its reputation is in a considerable degree still propped up by the favour of those who at first keenly espoused its cause. The notion of its efficacy having once been established

in their minds, they have neither inclination, nor perhaps time, to repeat the process of inquiry anew. Nay, may we not presume that the desire of preserving consistency prevents some from deserting an old and beloved friend, though they perceive that he is fallible like other men? Cold-bathing does not stop the progress of Fever with more certainty than the boasted use of emetics. Nay, bathing the feet in warm water, and a draught of warm gruel on going to bed, will, in most cases, as effectually stop a Fever as either of them. Washing a patient, however, occasionally, with due attention to his own feelings, has certainly a powerful effect in diminishing, for a little while, the uneasy sense of heat, and thus rendering a feverish complaint more easily borne.

It is worth remarking in this place, how changeable are the opinions of medical men. Fever is now considered by many practitioners as belonging to Dr Cullen's class of Phlegmasia; and this is a form of disease in which Dr Currie was rather afraid to recommend his favourite remedy.

The other remedy for Fever is blood-letting. Like cold-bathing, it is no new discovery, neither is its first employment in Fever of recent date. Of late years, however, it has been more extensively used, and the very respectable testimonies that are adduced in its favour from many able practitioners in most quarters of the world, surely entitle it to our most serious consideration.

It is to be observed, however, that the more extended use of venesection in the present day, is, in part at least, founded on a theoretical opinion. There is an association between inflammation and the taking away of blood, firmly established in our minds. The existence of the one is thought necessarily to infer the employment of the other. Indeed, the truth is, that no fact in the round of medical practice seems more unquestionably established than the efficacy of blood-letting in the cure of inflammation. I am not disposed to dispute the truth of this position ; but we live in a sceptical age. Twenty years ago, no fact seemed better established than this, that syphilis could not be cured without mercury. We would not believe the testimony of the very many res-

pectable physicians whose writings were collected by Luisinus Aphrodisiacus, and afterwards edited anew by Boerhaave. Of late years, however, we began to doubt; and now our doubts have grown into absolute disbelief; and indeed we have direct testimony to assure us, that syphilis undergoes a natural cure, although it be not denied that mercury accelerates the process.

If we attend to what may be called the natural history of inflammation, we shall find several facts which will serve to give us very important practical information.

We have to remark, then, that in a great many cases inflammation undergoes a natural cure. It does not always change the texture of the part which is its seat, it terminates without any dangerous effusion of fluid, it subsides without the formation of pus, it disappears without gangrene or sphacelus; if it sometimes ends in the adhesion of parts naturally separate, this adhesion often produces no inconveniency. In such cases the abstraction of blood appears at least unnecessary.

Again, inflammation terminates fatally in a variety of ways, even although venesec-

tion is employed with judgment and vigour. In many cases nothing can arrest its progress.

Here, then, we have one class of cases in which blood-letting is unnecessary, and another class in which it is unavailing. These facts very considerably circumscribe the range of its utility, and teach us to entertain a chastened confidence in the power of the remedy.

I have said that the testimony in favour of blood-letting in Fever is very respectable and extensive, and it may seem very absurd that any thinking person should call it in question. Yet I must say, that I think we ought to scrutinize this testimony before we put explicit confidence in it. There is at least a possibility that circumstances may be discovered, which may lead us to hesitate in giving implicit credit to it.

We know nothing of the nature of Fever certainly, nor of the *modus operandi* of the remedies that are said to cure it. All our knowledge that can be brought to bear on this subject is derived from testimony, and originates in belief.

Though testimony is one of the surest sources of knowledge, yet it certainly sometimes leads us astray, even in cases where the witnesses speak truth, and have no intention to deceive. The witness, and particularly the medical witness, is seldom content with giving his simple testimony. He builds upon the facts to the truth of which he testifies, and pushes his inferences farther than the facts upon which he founds them can legitimately carry him. It is not his testimony then that deceives us, but we deceive ourselves by following him in his conclusions.

This is eminently the case with respect to the subject now under consideration. We must assent to the truth of the assertions, that the sick specified by the numerous advocates for blood-letting were relieved by this operation, and on that account we cannot withhold our assent to the usefulness of the practice of taking away blood in certain cases of Fever. But the testimony carries us no farther. The same witnesses bring evidence, which appears also irresistible, to convince us that the efficacy of this remedy is of very limited ex-

tent. They assure us, that in a great many cases it did no good. One unsuccessful case counterbalances a considerable plurality of others, in as much as we know most assuredly that Fever often disappears without the aid of venesection, or without the aid of medicine at all; nay, even under the application of remedies, which the patronizers of venesection consider as certainly pernicious.

Dr Thomas Mills of Dublin has given us a useful Work, written with the express intention of proving the usefulness of bleeding in the cure of Fever. He has there recorded the cases of forty-seven patients who had Fever, with inflammation of the head, lungs, liver, and stomach. Notwithstanding the use of venesection, twelve of these patients died, which is a proportion of rather more than one in four. Now, although we were to admit that all these cases would have terminated fatally without venesection, we surely cannot say that a practice is very successful that saves only three patients in four. I am not sure that the mortality in the plague, left to itself, is in general very much greater. But from what we know of inflammation

and its spontaneous cure, I think it would be rash to say certainly that all the thirty-five patients, whom Dr Mills probably thinks he saved, would have died had they not been blooded.

Dr William Burnet has favoured us with a history of the Mediterranean Fever, and has endeavoured to teach us, that blood-letting is the principal remedy on which we are to depend for its cure. He has given us detailed accounts of twenty-five patients who were treated either by himself, or by his friends who entertained notions respecting the efficacy of blood-letting similar to his own. I do not see why these cases are brought forward, unless it be to establish the efficacy of venesection, or to show by dissection the inflammatory nature of the disease. The dissections surely establish the last mentioned fact; but the other seems in no respect to be corroborated. No less than one-third of the cases reported were fatal; yet all these patients were blooded. It would surely be building too much on our credulity to suppose, that the history of these

cases should convince us of the utility of venesection in the cure of Fever.

Dr Robert Jackson has written a valuable book on Fever. He is an advocate for venesection in the beginning of what is called Endemic Fever, and has given us twenty detailed cases of that disease. In these cases, the practice recommended by him was not more successful than what took place under other physicians. Of these twenty cases, no less than nine are said to have died.

We have also very strong testimony in favour of venesection in dysentery. Let us look a little at this testimony. In an elegant little work by Dr Somers, which would not have disgraced the classical pen of Sir George Baker, there is an account by one gentleman, of thirty dissections of persons dead of dysentery. These dissections certainly prove the point which he wished to establish, viz. that dysentery is a disease connected with local inflammation. But, with all due deference to the judgment of others, I cannot consider that as a powerful remedy which gives an opportunity of inspecting so many bodies to one individual. It

may, indeed, be the best remedy we have ; but, alas ! it is a remedy that fails us when we have most need of it.

After hearing witnesses give the same evidence, apparently so ambiguous, to the efficacy of blood-letting, it is almost unnecessary to say, that in order to come to the knowledge of the truth, we must have recourse to other sources of information.

In Medicine we are chiefly guided by the experience of individuals. We imitate the practices of a man whose opportunities have been ample, and of whose judgment and integrity we entertain a good opinion. Some, indeed, endeavour to found their practice on physiological knowledge ; but most people hesitate to trust to this foundation, and the appeal is generally made to experience at last. This experience must be that of individuals in the first instance ; but how little even this is to be depended upon, the small progress comparatively made in the cure of disease plainly shows. Perhaps the following statement may farther illustrate the truth and importance of this remark.

Dr Mills, in his book before referred to, informs us, that there were admitted into

the Fever-Hospital, Cork-street, Dublin,
from 14th of May 1804 to 4th of January
1812, inclusive,

| | | |
|--------------------------------|---|-------|
| Fever Patients, | - | 9195 |
| Discharged cured, | - | 8389 |
| Died, | - | 740 |
| 5th Jan. 1812, Remained in the | | |
| House, | - | 66 |
| | | <hr/> |
| | | 9195 |

The proportion of deaths, then, was nearly
1 in 11.

As this table contains the result of the
experience of seven years, and as the num-
bers are very considerable, it does not ap-
pear rash to take it as a common standard
of the efficacy of the judicious management
of Fever in Dublin.

Dr William Stoker, in his judicious Work
on Fever, which he treated without the gene-
ral use of blood-letting, has given separate
tables of the result of a considerable number
of sick treated by Dr Mills, and by the other
Physicians of an Hospital in Dublin. From
these lists he infers that Dr Mills was a less
successful practitioner than his brethren—
they having lost only 1 in 12, while it is al-

leged that he lost 1 in 11. Dr Mills has, however, called in question the accuracy of these tables, as far as he is concerned.

Placed in circumstances where it is impossible to examine the documents on which these calculations are formed, and having the result of seven years observation in 9195 cases before us, I do not think it will be injurious to the reputation of any of the Dublin Physicians, if we put them all on the same footing, and conclude they were equally successful, and did not lose more than 1 patient in 11.

We find, however, that the accounts given of individual experience are very far different from this; and the consideration of these accounts will, I doubt not, fully show how little useful information is to be derived from this source.

Dr Mills has given us, as above mentioned, forty-seven detailed cases of what he considered as Fever, and he lost 1 in 4.

He farther tells us, that at the Hospital in Cork-street, in 1810, of 209 patients who were blooded he lost 1 in 23.

In St George's Dispensary, of 91 patients who were blooded he lost 1 in 25.

In another place he informs us, that of 504 patients blooded, he lost 1 in 28.

Finally, in the table of 237 cases which he has printed in his book, he marks six deaths, one of which he openly affirms was owing to the neglect of venesection. Here, then, even counting this mismanaged case, he only lost 1 in 39.

Dr Stoker, as we have seen above, makes the success of the practice at the Hospital to be superior to that of Dr Mills, and places the loss at 1 in 12. When he details his own individual experience, however, we find a very different account. Dr Stoker has detailed 96 cases of Fever, and of these it appears that only one died.

Here we have very different results. This we must attribute to the nature of the particular cases which it fell to the share of the different gentlemen to treat ; but as we have no certain gauge to measure the violence of single cases, it must appear that individual experience is of less practical utility to others than has commonly been supposed.

In all cases, where it can be applied, the best way of coming to a sure conclusion respecting general truths, is to follow the me-

thod which has been so useful in fixing the duration of human life, and thus rendering the value of annuities a practical and useful art. This I attempted to do in my former paper, and I shall now endeavour to do the same here in examining the real value of the use of blood-letting in abridging the duration, and accelerating the cure of Fever.

The facts concerning the natural history of Fever which we possess, are by far too few to enable us to come to any accurate knowledge of its origin and course. It is indeed to be feared, that it will be long before we obtain the facts required, more particularly those relating to its course. It is seldom that mankind suffer disease without some attempt to relieve themselves from it. Indeed, supine suffering in any ailment, even although it is known to be incurable, is not to be commended ; but were it possible to get accurate histories of a sufficient number of cases of Fever, where the disease was left wholly to nature, I am persuaded a very considerable advance might be made in perceiving more distinctly the efficiency of the means of shortening, perhaps,—certainly of

alleviating it, and rendering it less dangerous.

I have collected a number of cases of Fever, and have endeavoured to note some particulars respecting the course of this ailment. In my former paper I gave the result of my inquiries in 280 cases. Dr Stoker has furnished us with 96 cases, which are detailed, and 1773, which he has thrown into the form of a table; Dr Mills with 237; Dr Burnet, of patients under his care, and from other sources, 95; Dr Robert Jackson with 30; and, by the kindness of the Managers and Physicians of the Royal Infirmary, I have got possession of 127. In all 2648 cases.

The cases supplied by Dr Burnet, and twenty of those given by Dr Jackson, do not indeed belong to the Fever treated by the others. This, at least, is the more common opinion: they are, besides, selected, and on that account were probably published to establish a particular point. These considerations may be thought to render it doubtful whether they ought to have been brought forward. It is to be remembered, however, that the Mediterranean, endemic,

or remittent Fever, has certainly some affinity to Typhous, though it may not originate in the same source, nor be propagated in the same way. It surely then may be made to illustrate some points that appear common to all febrile diseases. It is only in this way that I mean to employ the information which they convey.

From an examination of these cases, something may be made out respecting the duration and course of Fever. My readers will find, perhaps, an unexpected uniformity in Fever, in this respect, at different times, in different places, and under a treatment considerably different.

During the ten days from the fifth to the fourteenth inclusive, the terminations are,—

| | | |
|-------------------------------------|---|------|
| In my table, Annals of Medicine, of | | |
| 280 cases, | - | 198 |
| In Dr Stoker's table of 1773 cases, | | 1410 |
| Dr Stoker's own cases, 96, | - | 61 |
| Dr Mills' own cases, 237, | - | 183 |
| Infirmary cases 1817, 127, | - | 83 |

By reducing these numbers to decimal parts, we find that the proportions are not far different. Thus—

| | | | |
|-------------------|---|---|------|
| 198 in 280, gives | - | - | ·707 |
| 1410 in 1773, | - | - | ·788 |
| 61 in 96, | - | - | ·635 |
| 183 in 237, | - | - | ·772 |
| 83 in 127, | - | - | ·653 |

May it not be legitimately inferred, that if the exact time of attack and convalescence had been noted, were that always possible, there would have been a still nearer sameness in the result?

There is one circumstance appears from the examination of these lists, which, although it does not immediately bear on our subject, seems not unworthy of notice. It relates rather to the social than medical history of that part of the community which is benefited by Hospitals: it is, however, so far medical, that it shows that the Physicians of Hospitals are upon an equality in what relates to the advantages to be expected from the early application of medicine in the cure of Fever.

Dr Stoker and Dr Mills have both noticed the day of the disease on which a patient came under their care. This is dis-

tinctly stated in 84 of Dr Stoker's cases, and in 231 of Dr Mills'.

I have summed up the amount of all the days, and taken an average, and I find, that in Dr Stoker's cases the mean length of illness, before admission, is $7\frac{1}{2}$ days.

In Dr Mills' cases, - $7\frac{1}{2}\frac{7}{3}\frac{7}{1}$ days.

In the Infirmary cases 1817, 8 days and a fraction.

In the table of 280 cases given in the Annals of Medicine, the time of sickness before admission into the Hospital appears to be only $6\frac{1}{2}$ days. Upon a little reflection, however, it will appear, that all the four accounts are very nearly in unison in this respect. In that table the number of admissions, on the first day of Fever, is stated to have been 22. This must be a mistake. It is quite certain, that in a very few cases only, Typhous Fever can be known on the first day of attack; besides, it is contrary to common observation, that, in ordinary cases, any person, on finding himself unwell, immediately removes himself from his home, without waiting at least one day to see whether he shall not be better. If

then we throw the number said to have been admitted on the first day into the second or third, we shall find, that the average days of sickness correspond with the other accounts, and is $7\frac{4}{10}$ days.

It is generally admitted, that epidemic Typhous differs considerably at different times, and in different places, in point both of duration and severity. The knowledge of this fact must have some effect on the judgment we form of the value of any particular mode of practice. Provided that two epidemics are of the same kind in other respects, it is evident that the success of medical practice must appear greater in the Fever of shorter duration, because the number of early convalescences must be greater.

It appears, that the mean duration of the Fever at Edinburgh, about the end of the last century, was - 13 days.

Of the Fever treated by Dr Stoker

and Dr Mills, - 11

Of the Fever at Edinburgh, 1817, $12\frac{1}{2}$

Keeping this part of the history of Fever in view, we shall endeavour to compare the success of Dr Mills and that of other Phy-

sicians, by finding out the mean length of time that their respective patients were under medical treatment before convalescence.

Dr Mills has been very careful to mark the day on which the convalescence of his patients commenced. Dr Stoker has also marked the same thing with respect to the sick under his care, but not always with the same apparent precision. In the cases collected at Edinburgh, perhaps the precision is still less. Indeed, the Physicians there seem to report only such circumstances as may be useful in enabling them to conduct the cure of their patients, without the view of making use of their observations afterwards for specific purposes.

I have cast up the mean time of medical treatment before convalescence began, and I find that at Edinburgh, in the 280 cases so often mentioned, it amounted to between four and five days.

| | | |
|------------------------|---|------------|
| At Edinburgh, 1817, | - | 4 days. |
| In Dr Stoker's cases, | - | 81½ hours. |
| In Dr Mills' patients, | - | 76 hours. |

From these results it would appear, that Dr Mills' practice had a more decided influence on Fever, than the Edinburgh practice, or that of Dr Stoker. But supposing this granted, it does not follow that we must impute this advantage to blood-letting alone. Dr Mills tells us, that he administered a brisk purge every morning. If medicine has any influence on Fever then, we must surely give purging a share of the praise.

It is to be remembered, however, that although Dr Mills' practice appears superior in the respect in which it has been viewed ; yet in others it does not manifest any superiority. Thus, if we consult the table of Hospital practice, showing the days on which Fever terminated, we shall find, that the Physicians who had the charge of the Hospital at that period, cured 788 in the space of time in which Dr Mills cured 772.

Perhaps some farther light will be afforded by considering another thing connected with the practice of medicine in Fever. It is asserted, that the efficacy of medicine is in proportion to the earliness with which it is applied.

Dr Thomas Young of London has done me the honour to notice my former little performance, and has even thought the positions there laid down to have so much the semblance of truth as to deserve to be refuted. May I request him to examine the statements I am about to make, and appeal to his candour to say, whether I have not the appearance of truth still on my side.

Dr Young, in his valuable work entitled “An Introduction to Medical Literature,” &c. has made the following observations: —“In fact, the results of about 300 cases of Fever admitted into the Infirmary in question, may be very simply stated in this manner. The mean duration of the whole disease was 12 days; the mean time of admission was between the 6th and 7th day: but the mean duration of those cases which were admitted at the commencement of the disease, was somewhat less than 9 days. Hence it may be inferred, not only that the duration was shortened at least 3 days by the early employment of medical agents, but also that this difference was the effect of a difference of only $3\frac{1}{2}$ days in the time

of the whole treatment ; since the remedies were employed for 9 days in the one instance, and for $5\frac{1}{2}$ in the other : consequently, if these $5\frac{1}{2}$ days had been suffered to elapse without medical treatment, the whole period of the disease would have been lengthened 4 or 5 days by the omission, at least if we adopt the simplest supposition respecting the proportion of the cause and effect ; and that the natural duration of the disease thus determined would have been about 16 days instead of 12 ; so that the means employed must be allowed to have a claim to the merit of reducing the duration of Fever from about 16 to 9 days.”

With much respect and deference to Dr Young, I would propose the following facts to his consideration. When we take a medium of various durations, it is certain that the number of these individual durations, shorter than the medium, must be greater than the number that exceed it. Thus, if we find the mean duration of Fever to be 12 days, we are certain that more cases occur of a shorter duration than 12 days

than what exceed 12 days. This is the same whether the patients die or live.

I farther request him, with his usual candour, to consider, whether it be not an indisputable fact, that in every epidemic, a certain proportion of the sick would recover before the mean period, whether they employed medical agents or not.

If these things be really true, it appears to me indubitable, and I trust it will appear so also to Dr Young, that the fact of the shorter duration of Fever in cases where medicine was early employed, do not satisfactorily prove the truth of the conclusions he has drawn in the above quotation.

But I think I can adduce pretty strong evidence, that medicine is not efficacious in proportion to the early period of Fever in the cure of which it is applied ; or, in other words, that a Fever of two or three days duration requires as long an application of medicine as a Fever much farther advanced. With this view I have constructed the following tables, which I trust will be easily understood.

TABLES showing the proportionate duration of Fever, under the effects of Medicine applied at different periods of the Disease.

| | | | | | | | | | |
|--|--|-----|------|------|------|-----|-----|-----|------|
| Dr BROWN'S cases of a Fever of 15 days mean duration. | NO. of days ill before medicine was used. | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |
| | NO. of Patients. | 17 | 52 | 59 | 59 | 25 | 20 | 35 | 7 |
| | NO. cured on or before the 9th day of treatment. | 11 | 22 | 29 | 35 | 18 | 15 | 29 | 6 |
| | Proportion to the whole in decimal parts. | .64 | .687 | .745 | .846 | .72 | .75 | .8 | .857 |
| Dr MILLS' cases of a Fever of 11 days mean duration. | NO. of Patients. | 14 | 9 | 37 | 44 | 22 | 45 | 18 | |
| | NO. cured on or before the 5d day of treatment. | 10 | 5 | 29 | 26 | 17 | 55 | 10 | |
| | Proportion to the whole in decimal parts. | .71 | .55 | .78 | .59 | .76 | .76 | .55 | |
| | NO. cured on or before the 9th day of treatment. | 15 | 8 | 35 | 41 | 21 | 41 | 17 | |
| | Proportion to the whole in decimal parts. | .92 | .88 | .94 | .95 | .95 | .95 | .94 | |
| Dr SPOKER'S cases of a Fever of 11 days mean duration. | NO. of Patients. | 4 | 10 | 8 | 15 | 10 | 7 | 15 | 2 |
| | NO. cured on or before the 5d day of treatment. | 2 | 9 | 6 | 7 | 6 | 6 | 9 | 1 |
| | Proportion to the whole in decimal parts. | .5 | .9 | .75 | .55 | .6 | .87 | .69 | .5 |
| | NO. cured on or before the 9th day of treatment. | 4 | 9 | 7 | 12 | 9 | 7 | 11 | 2 |
| | Proportion to the whole in decimal parts. | .9 | .87 | .92 | .9 | | | | .84 |

A simple inspection of these Tables puts it beyond a doubt, that medicine was as effectual when applied as late as the 6th, 7th, 8th, or 9th day of the disease, as when more early used.

I have taken no notice of the effect which the proper administration of medicine has in preventing death from Fever. I have not been able to procure what appeared to me sufficient data. I have already hinted at the insufficiency of the experience and testimony of individuals. It would surely be unjust to estimate the value of medicine, and the success of the practice of Dr Mills, from the result of his 47 cases, of which he lost one-fourth ; or of Dr Burnet's 25 cases, of which one-third died ; or of Dr Jackson's 20 cases, near one-half of which proved fatal. In all these instances it would be almost as illiberal to take them as a test of the success of the gentlemen who treated them, as it would be to infer that Mr Brien lost all his patients, because Dr Burnet has recorded ten fatal cases in which venesection was not employed, without any instance of recovery to balance the dismal account. On the other hand, it surely would not be wise to take Dr Stoker's account as a standard of his success. I think few men in long continued practice will be found who cannot say that they have attended 96 patients in Fever who recovered from it ;

but neither Dr Stoker, nor any one else will be found who can assert, that the ordinary proportion of death from Fever in his whole practice has only been 1 in 96. It is from a great number of cases, faithfully recorded without selection, that we can come to any knowledge of this part of the natural history of Fever.

After a due examination of the documents I have produced, and the considerations I have suggested, I surely may say that we are warranted to withdraw somewhat of the confidence we are too easily led to entertain of the efficacy of any particular practice in the cure of Fever. We may safely doubt whether we can stop the progress of this disease by any means that have hitherto met the approbation of the Medical Faculty. With respect to myself, I must declare, that the sentiments expressed in my former paper have received additional confirmation, and that the fact really is, that however much Medicine may conduce to the safe conduct of this disease, it has not hitherto interrupted its course, and that we have no evidence to convince us that our knowledge of Fever is much increased, or

our controul over it extended, since I last ventured to appear before the Public.

Although Dr Mills, and other eminent Physicians, have not proved to my conviction that they shortened the duration of Fever by the general employment of blood-letting, yet they certainly deserve much commendation for what they have done.

Many of us had almost forgotten that venesection, to a moderate extent, may be safely used in the cure of Typhous or nervous Fever. There is no evidence, so far as I can perceive, that any of the patients of Dr Mills incurred harm from the abstraction of blood. With respect to the practice of others, the means of judging have not been so distinctly afforded.

This fact leads us back to the days of Sydenham, whose vigorous understanding and sober judgment served much to banish alexipharmics in the treatment of Fever. After this, for many years, Physicians seem to have been under the guidance of the sober influence of the humoral pathology, which indeed led to the employment of much medicinal matter, but rather discouraged the energetic means of cure that

were afterwards introduced. Some time after this, the doctrine of putrefaction was considerably extended ; and the Medical world was busied in searching after antiseptic remedies. Our minds were then intoxicated with an undue extension of the nervous pathology, and the dreams of the Brunonian School. Of late, local inflammation has been a prominent object of contemplation among Physicians, and has had a principle share in extending the use of the remedy which Dr Mills has so generally employed. His practice has shown, without any intention on his part, that the loss of blood, in moderate quantity, is a matter of no very great importance, and is neither productive of the very bad consequences that were often apprehended by those who placed all danger in debility, nor of great and certain good effects, which they, who consider inflammation as the almost only thing to be feared, expect to derive from it.

It would be unjust to pass over without acknowledgment the obligations we are under to the advocates of the various systems which have at different times occupied the minds of Physicians. It is almost im-

possible to imagine that the persons who employed the means of curing Fever founded upon these systems, could fail to perceive the very bad effects which each attributes to his antagonist, had these bad effects been really produced. This would be to claim for the present generation a superior degree of clear-sightedness which we can scarcely think it legitimately entitled to. The more probable explanation surely is, that the means employed under all the different systems of putridity, nervousness, debility, and inflammation, are not so powerful agents as their employers suppose ;—that they have not that effect in determining the ultimate result of a Fever, which is expected from them ;—that their operation may materially affect the present comfort of the individual, but do very little that can influence the final issue of his disease.

If these observations be just, the knowledge of them gives us very important advantages. While it does not oppose any obstacle to the farther investigation of the disease, or to the invention of more powerful means of cure, it teaches us to employ the means of relief which we do possess

more extensively, with more certainty and more uniformity for the advantage of the sick. It hinders us from fixing our attention on one single view of the nature of the disease, the justness of which we must hesitate to acknowledge, and calls us to contemplate symptoms with more earnestness, and to direct our endeavours principally to the removal of uneasy sensations, and restoration of disordered functions. Hence we may be delivered from the fear of moderate blood-letting in diseases called putrid—of the prudent use of wine in those termed inflammatory—of the cautious employment of opium in all cases where its soothing operation can allay uneasy feeling—of heat, where the comfort of the patient is increased by it—and of cold, where it brings agreeable sensation along with it. Nor need we dread the stimulant and putrescent properties usually attributed to different kinds of food. In this respect, we may safely abandon the patient, if not under delirium, to his own experience, or direct him more according to the principles of common observation, than according to rules of an uncertain philosophy.



